

# Detector challenges for proposed future 100 TeV hadron colliders

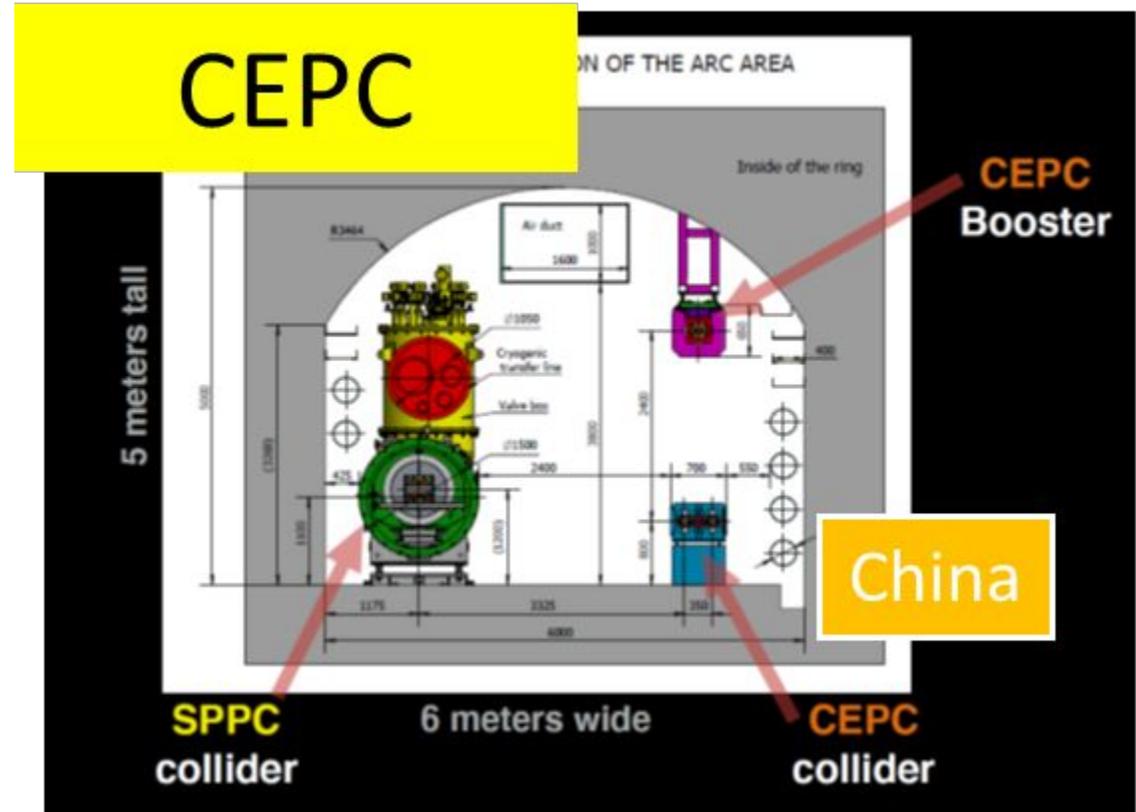
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U. Maryland

P5 Town Hall, BNL, April 2023



# SppC after CEPC : another path



I will use examples from FCC-hh, as I am part of that collaboration. The goal specs, the work required, and technical challenges would be quite similar.

# Yamamoto's view on magnet timeline

## Personal View on Relative Timelines

Timeline	~ 5	~ 10	~ 15	~ 20	~ 25	~ 30	~ 35
<b>Lepton Colliders</b>							
SRF-LC/CC	Proto/pre-series	Construction		Operation		Upgrade	
NRF-LC	Proto/pre-series	Construction		Operation		Upgrade	
<b>Hadron Collider (CC)</b>							
8~(11)T NbTi/(Nb <sub>3</sub> Sn)	Proto/pre-series	Construction		Operation			Upgrade
12~14T Nb <sub>3</sub> Sn	Short-model R&D	Proto/Pre-series		Construction		Operation	
14~16T Nb <sub>3</sub> Sn	Short-model R&D		Prototype/Pre-series		Construction		

**Note: LHC experience: NbTi (10 T) R&D started in 1980's --> (8.3 T) Production started in late 1990's, in ~ 15 years**

# 100 TeV hadron collider detector challenges

There are many interesting detector challenges that should be solved before FCC-hh detector construction begins, perhaps 20-30 years before its start, which will perhaps be in the late 2070's!

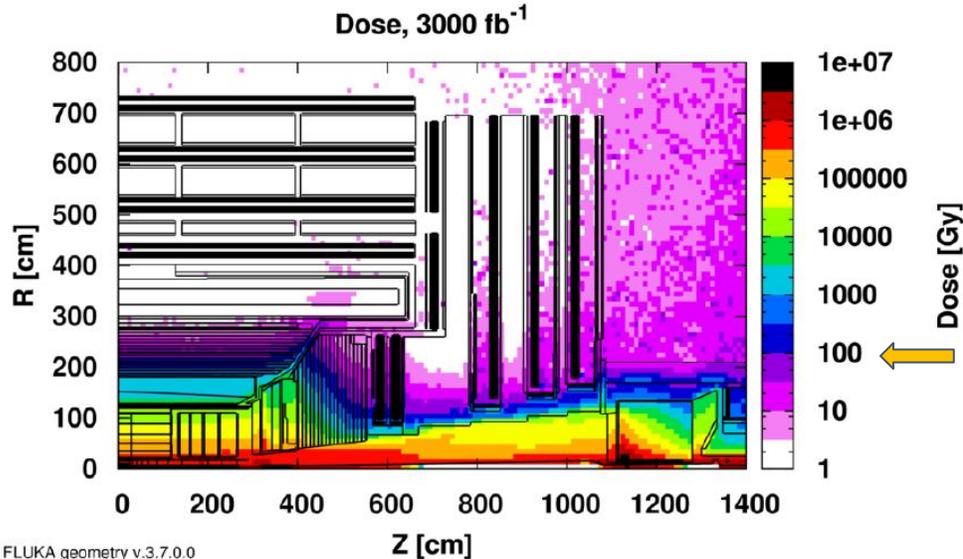
There will be detectors made for other experiments between now and then. Some of the necessary technology will be prototyped in these detectors. In this talk, I will concentrate on those that risk getting missed if we rely only on that for our prototyping. This has happened in the past.



# 100 TeV pp collider detector challenges: radiation

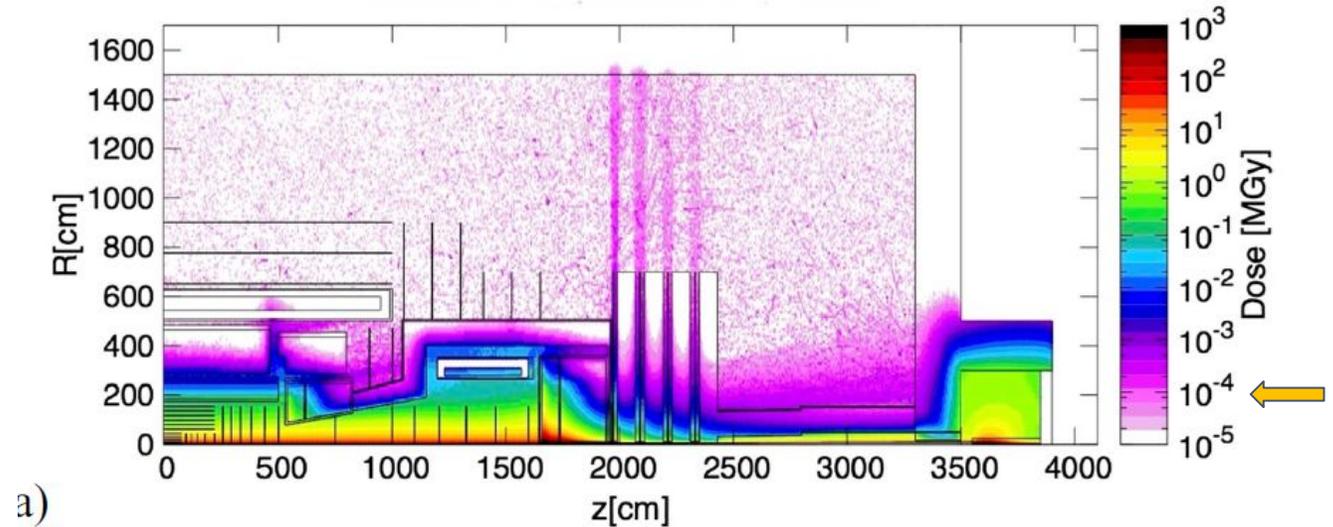
HL-LHC (CMS detector) 14 TeV, 3 ab<sup>-1</sup>  
-<https://cds.cern.ch/record/2020886?ln=en>

FCC-hh, 100 TeV, 30 ab<sup>-1</sup>  
<https://link.springer.com/article/10.1140/epjst/e2019-900087-0>



CMS FLUKA geometry v.3.7.0.0

Figure 1.15: Absorbed dose in the CMS cavern after an integrated luminosity of 3000 fb<sup>-1</sup>. R is the transverse distance from the beamline and Z is the distance along the beamline from the Interaction Point at Z=0.

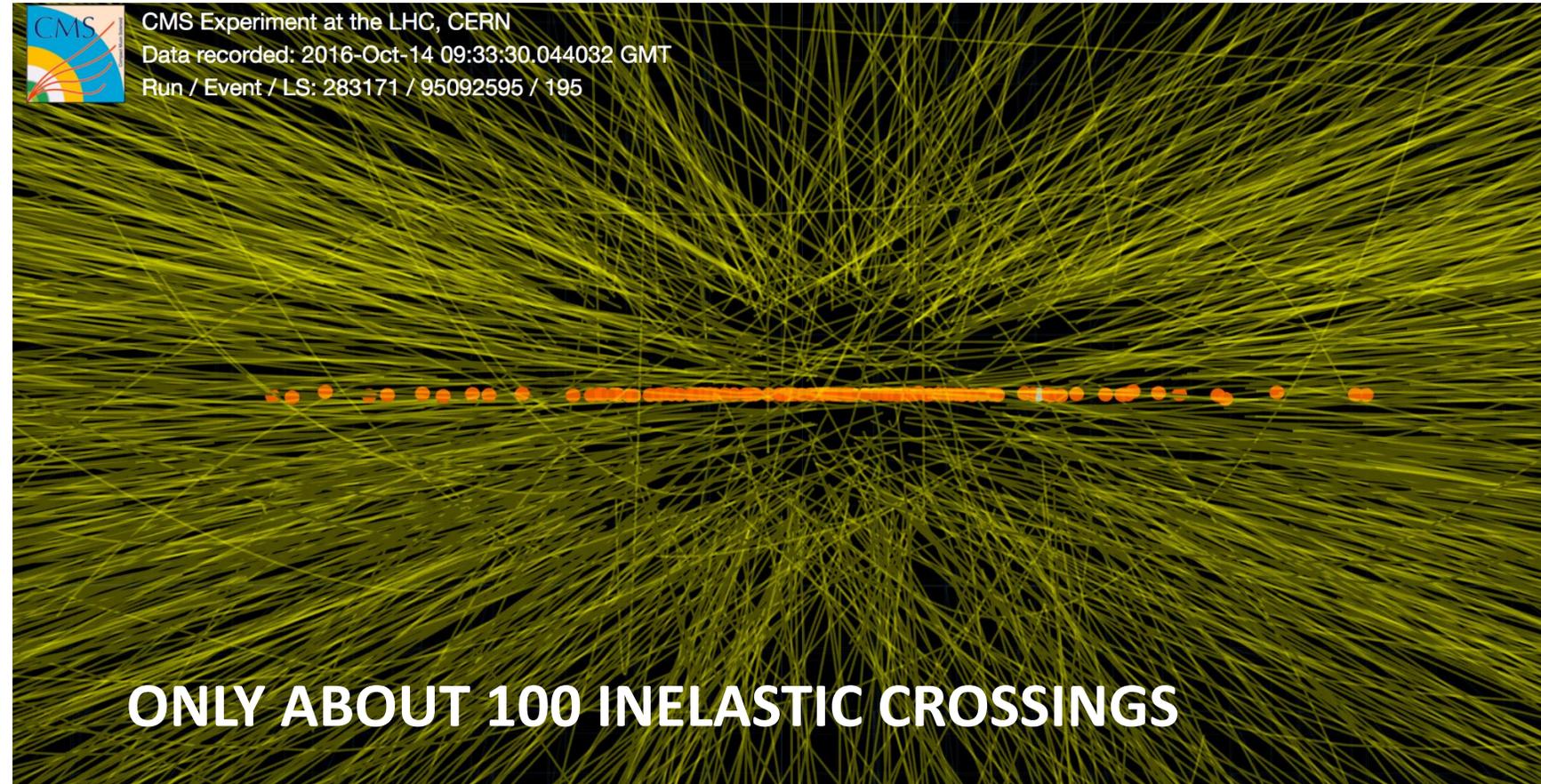


We do have extensive experience with intense radiation environments from our HL-LHC preparations, but this really is another world. Extensive R&D will be needed.

Activation at the end of run is an interesting challenge.

# 100 TeV pp collider detector challenges: pileup

- number of inelastic collisions per crossing:
  - LHC: 27
  - HL-LHC: 135 (5x)
  - **FCC-hh 1026**
- average of 20 b pairs per crossing
- average of 3 jets with  $p_T > 50$



The detector community has developed remarkable tools for pileup identification for HL-LHC. We need to go an order of magnitude beyond this for FCC-hh. May need track timing of 5 ps

# Strawman FCC-hh detector (an example)

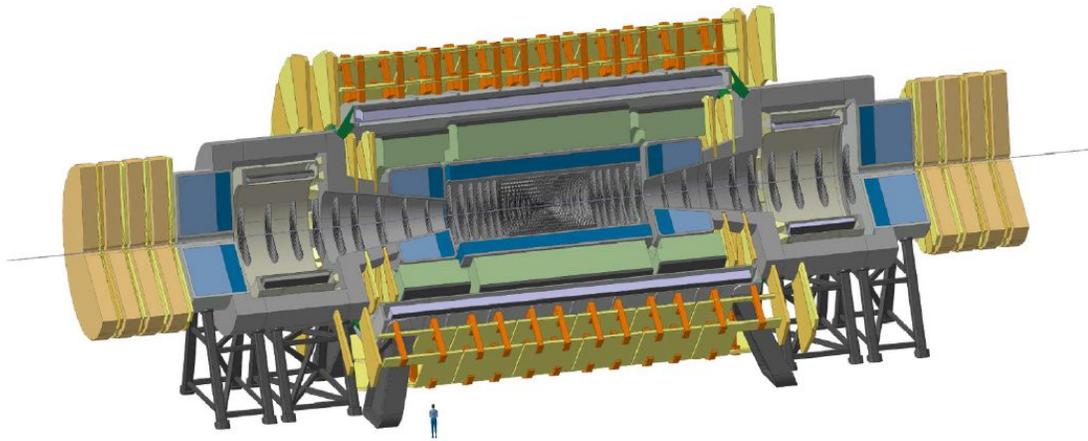


Fig. 7.1. The FCC-hh reference detector with an overall length of 50 m and a diameter of 20 m. A central solenoid with 10 m diameter bore and two forward solenoids with 5 m diameter bores provide a 4 T field for momentum spectroscopy in the entire tracking volume.

Important features in any FCC-hh detector (illustrated in this strawman)

- excellent 4-momentum resolution for charged and neutral particles to very high pseudorapidities ( $\sim 6$ )
- thick enough to contain the highest energy particles (12 lambda)
- radiation tolerant
- excellent pileup discrimination
- tracking resolution 10-20% at 10 TeV
- muon resolution 5% at 10 TeV
- excellent b tagging
- Calorimeter EM sampling term 10%, noise term  $< 1.5$  GeV including pileup
- HCAL constant term of 3%
- affordable

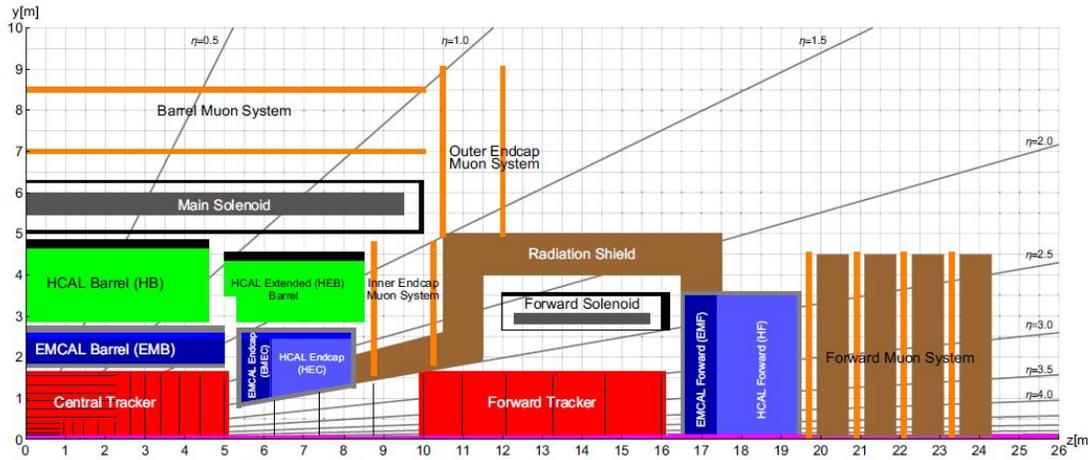
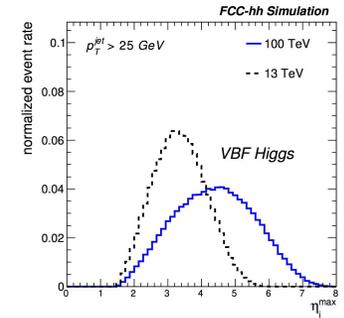
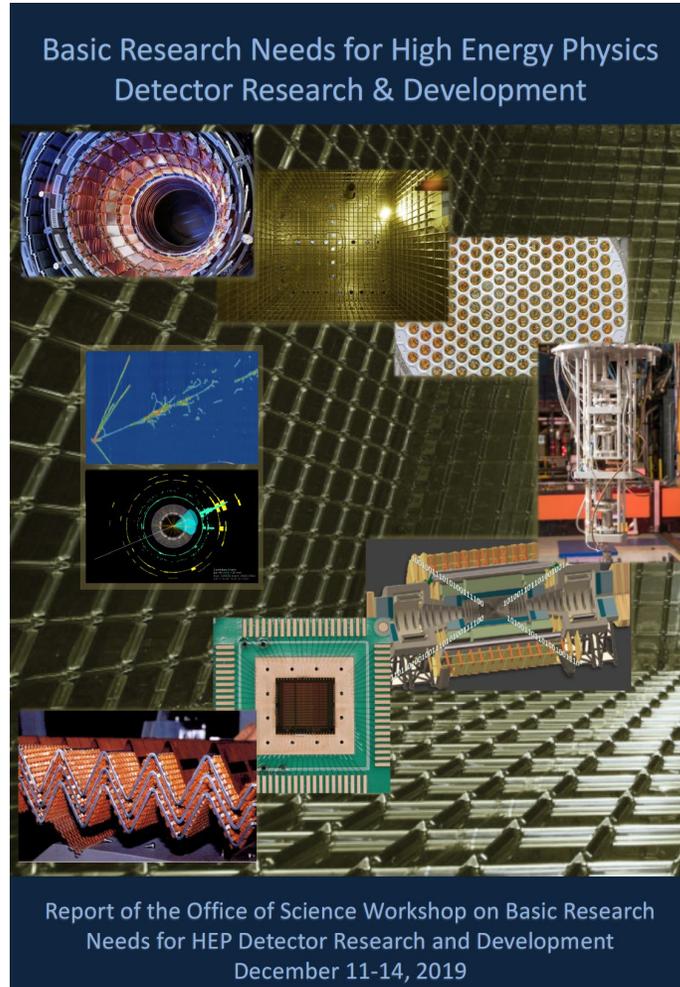


Fig. 7.2. Longitudinal cross-section of the FCC-hh reference detector. The installation and opening scenario for the detector requires a cavern length of 66 m, which is compatible with the baseline assumption of  $L^* = 40$  m for the FCC-hh machine.

# BRN and ECFA roadmaps

More detailed requirements for FCC-hh are laid out in the BRN and the ECFA roadmap.  
Also, please see the excellent talk by Martin Aleksa

<https://cds.cern.ch/record/2784893>  
<https://www.osti.gov/biblio/1659761>  
<https://indico.cern.ch/event/994685/>



# magnets

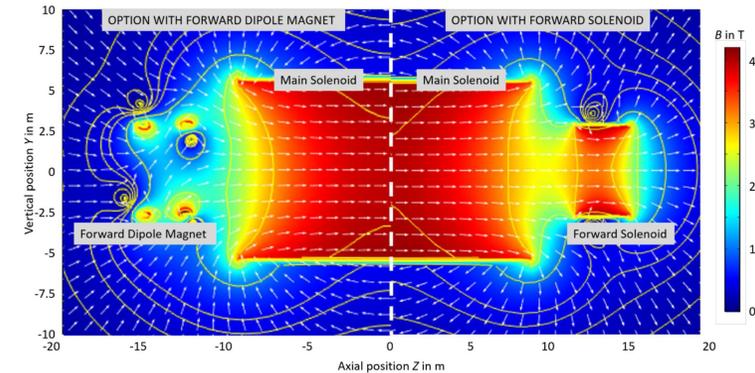
To get the required momentum resolution to high enough pseudorapidity, need an unprecedented magnet:

- ATLAS 2.7 GJ
- CMS 1.6 GJ
- FCC-hh: 13 GJ????

Martin Aleska at <https://indico.cern.ch/event/994685/>

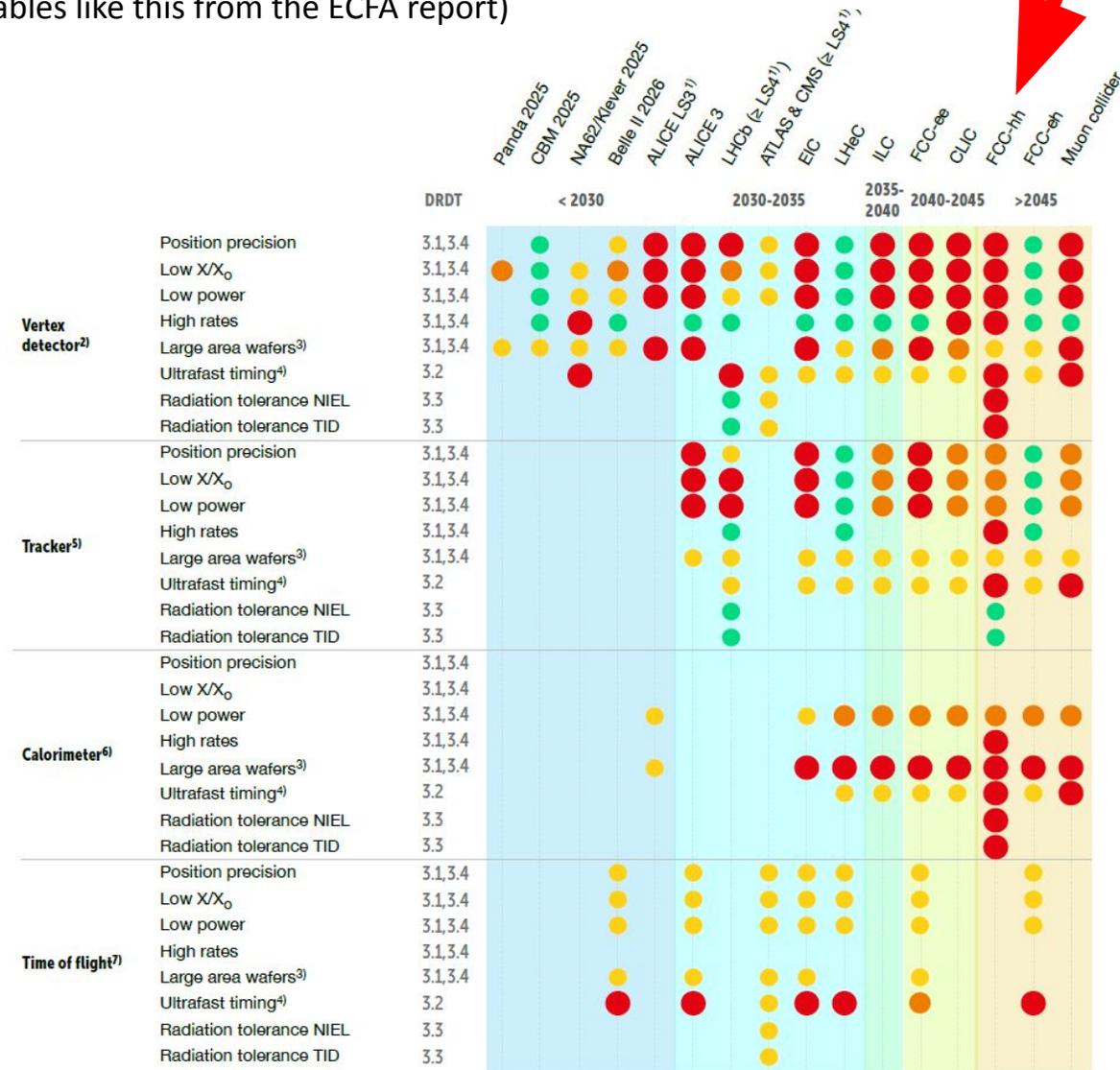
- New orders of magnitude of **stored energy!**
- **R&D needs (4T,  $r = 5\text{m}$ , length  $\approx 20\text{m}$ ):** Conductor development, powering and quench protection, coil windings pre-stressing, conduction cooling techniques and force transfer to cryostat and neighboring systems.
- **R&D needs** for the ultra-thin and radiation transparent solenoids: Study the limits of high yield strength Al stabilized NbTi/Cu conductor and its cold mass technology affecting the feasibility of the concept of such a challenging magnet.
- **Low material cryostats**, Al-alloy honeycomb or composite material (carbon-fibre)

Because the design of the experiment hall depends on the magnet design, this work needs to come early.



# Solid State Detectors

(Tables like this from the ECFA report)



● Must happen or main physics goals cannot be met ● Important to meet several physics goals ● Desirable to enhance physics reach ● R&D needs being met

For 100 TeV proton colliders, improvements needed for vertex detectors, tracking, calorimeter. Much R&D needed

high priority items for vertex trackers are:

- position precision
- thinness
- low power
- high rates

high priority items for calorimeters are

- ultra-fast timing
- radiation tolerance, rate capacity
- large area wafers

Synergies: good synergy with muon collider, except for rate and radiation requirements.

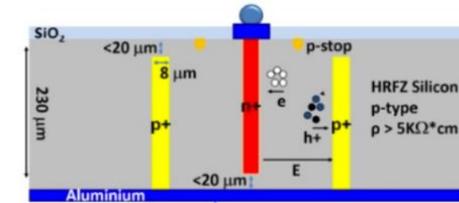
# Challenges for the Tracker – R&D Needs (TF3)

- **Radiation hardness:**
  - Radius > 30cm: Existing technologies are applicable
  - Radius < 30cm: Radiation challenge has to be solved
    - Ultra-radiation hardness of sensors and read-out chip
    - Up to  $10^{18} \text{cm}^{-2}$  1 MeV n.eq. fluence, TID of 300MGy
- **Timing of tracks at the <10ps level**
  - Either timing measurement of each pixel or dedicated timing layers
  - LGAD for timing O(30ps) achieved, ultra-thin LGADs  $\leq 10\text{ps}$ 
    - Improve rad. tolerance, now up to  $2 \times 10^{15} \text{n/cm}^2$  (esp. gain layer, admixture of doping elements)
    - Limited to relatively large cells due to inefficient collection at pad edges  $\rightarrow$  smaller cell sizes
  - 3D Pixel technology  $\rightarrow$  radiation tolerance up to  $3 \times 10^{16}$  neutrons/cm<sup>2</sup> demonstrated, timing O(30ps)
  - R&D on new technologies to achieve <10ps timing resolution
- **Low material**
  - **Monolithic designs with integrated sensor and readout** (e.g. MAPS)  $\rightarrow$  R&D on improving radiation hardness to make it compatible with **outer layers** of future tracker.
  - **Outer layers:** waver scale CMOS sensors have the potential to reduce power consumption and fulfill low-material budget requirement
- **Integration problems to be solved (TF7, TF8, TF3):**
  - Huge amount of data produced (1000TByte/s)
  - Power needs of sensors, FE-chips and optical links critical
    - $\rightarrow$  keep material for power lines and cooling under control
  - Low-mass detector system integration: This includes integrated services, power management, cooling, data flow, and multiplexing.
- **New sensor materials?** E.g. to work at room temperature?
- **Far future:** R&D on mass-minimized, or irreducible-mass tracker, namely, a tracker which mass budget is reduced to the active mass of the sensor

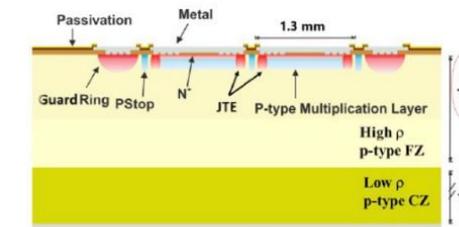
Parameter	Exp.	LHC	HL-LHC	SPS	FCC-hh	FCC-ee	CLIC 3 TeV
Fluence [ $\text{n}_{\text{eq}}/\text{cm}^2/\text{y}$ ]		$\text{N} \times 10^{15}$	$10^{18}$	$10^{17}$	$10^{16} - 10^{17}$	$<10^{10}$	$<10^{11}$
Max. hit rate [ $\text{s}^{-1}\text{cm}^2$ ]		100 M	2-4 G****)	8 G****)	20 G	20 M****)	240k
Surface inner tracker [ $\text{m}^2$ ]		2	10	0.2	15	1	1
Surface outer tracker [ $\text{m}^2$ ]		200	200	-	400	200	140
Material budget per detection layer [ $X_0$ ]		0.3% <sup>1</sup> -2%	0.1% <sup>1</sup> -2%	2%	1%	0.3%	0.2%
Pixel size inner layers [ $\mu\text{m}^2$ ]		100x150-50x400	$\sim 50 \times 50$	$\sim 50 \times 50$	25x50	25x25	$< \sim 25 \times 25$
BC spacing [ns]		25	25	$> 10^9$	25	20-3400	0.5
Hit time resolution [ns]		$< \sim 25 - 1\text{k}^2$	$0.2^{**1} - 1\text{k}^2$	0.04	$\sim 10^{-2}$	$\sim 1\text{k}^{***}$	$\sim 5$

\*) ALICE requirement \*\*\*) LHCb requirement \*\*\*\*) At Z-pole running \*\*\*\*\*) max. output rate for LHCb/high intensity flavour experiments: 300-400 Gbit/s/cm<sup>2</sup>

Table from EP R&D Final Report (CERN-OPEN-2018-006)



3D Pixel ([arXiv:1806.01435](https://arxiv.org/abs/1806.01435))

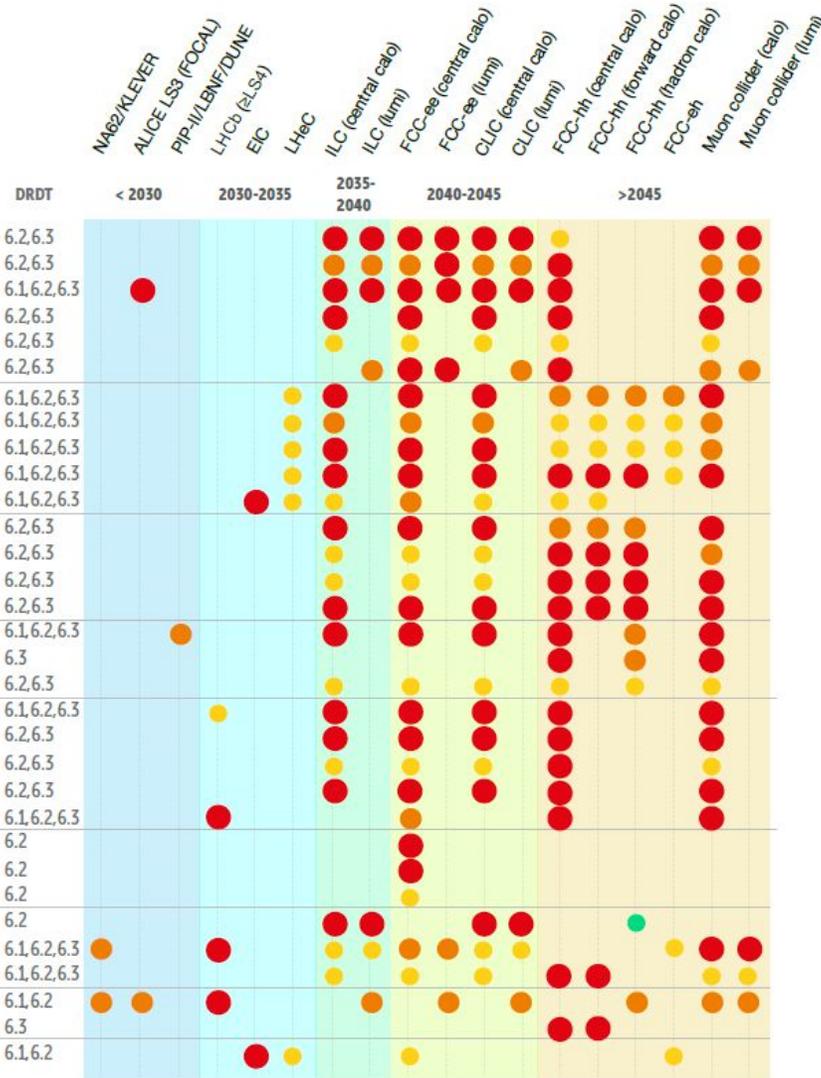


LGAD, see e.g. [talk by S. Grinstein](#)

# Calorimetry



For 100 TeV proton colliders, so much R&D need, 3 columns are needed, for different regions. EM calorimeter especially challenging. Known rad-hard calorimeters are liquid argon (will benefit from granularity improvement R&D for FCCee) or cherenkov-only detectors (but their resolution is not adequate) Silicon (but will it saturate? extremely high granularity?) Calorimeter segmentation must be factor 4 smaller than HL-LHC calorimeters



● Must happen or main physics goals cannot be met    ● Important to meet several physics goals    ● Desirable to enhance physics reach    ● R&D needs being met

Synergies: many with muon collider

# Challenges for Calorimetry – R&D Needs (TF6)

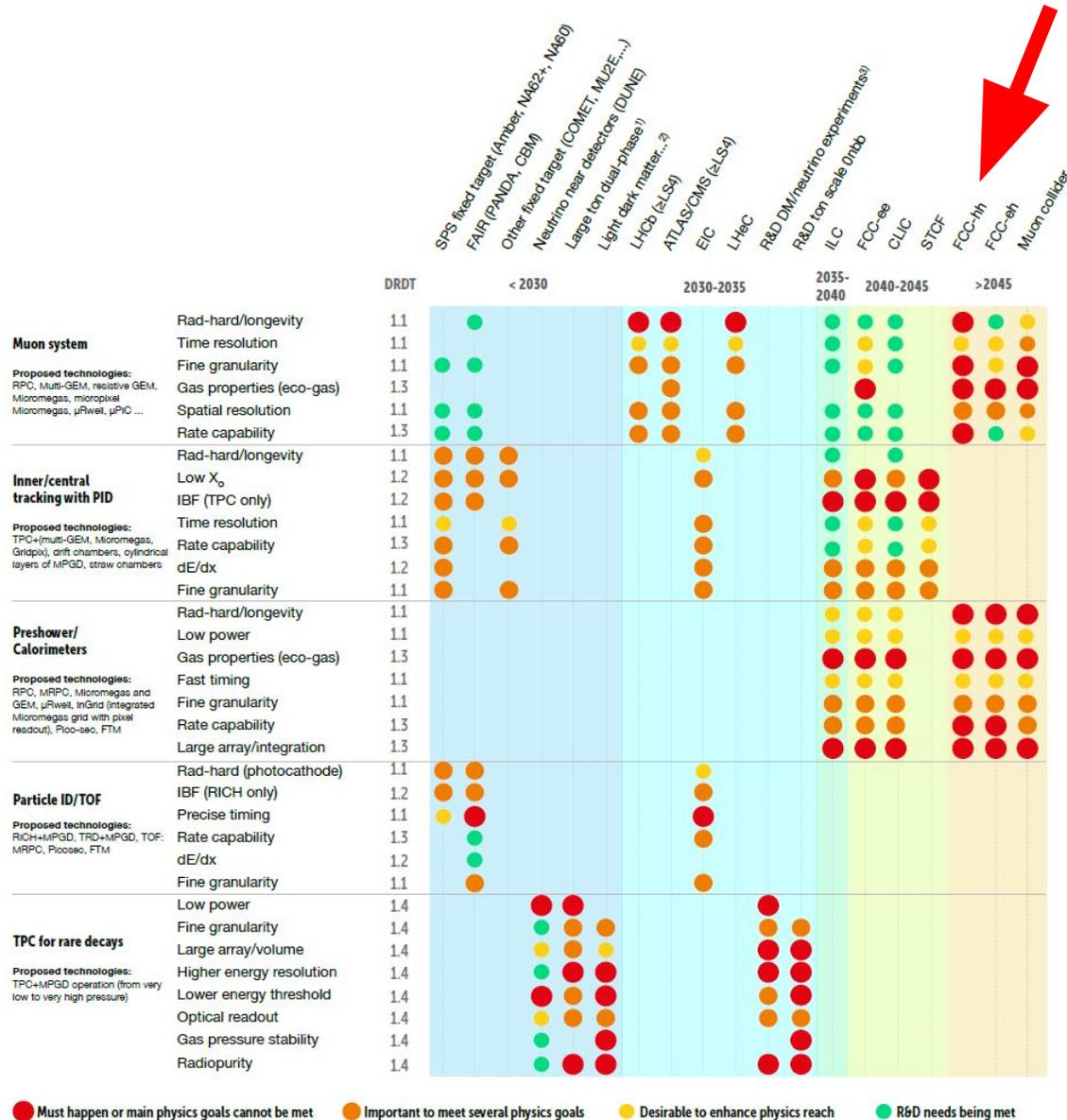
- **Radiation hardness:**
  - **Forward calo:**  $5 \cdot 10^{18} \text{ n}_{\text{eq}}/\text{cm}^2$ , 5000MGy
    - Noble liquid calorimetry – intrinsic radiation hardness (of active material), other components (e.g. read-out electrodes!) need to be well chosen and tested. Electronics well shielded behind calorimeter outside the cryostat.
  - **Barrel and endcap ECAL:**  $2.5 \cdot 10^{16} \text{ n}_{\text{eq}}/\text{cm}^2$ 
    - Noble liquid calorimetry,
    - Si as active material maybe possible in the barrel ECAL → need to increase radiation tolerance by factor 3-5
    - Inorganic crystal scintillators: e.g. Cerium doped LYSO
    - SPACAL-type calorimeter with crystal fibres (e.g. YAG or GAGG) → need to increase radiation tolerance by factor 5
  - **Barrel HCAL:**  $4 \cdot 10^{14} \text{ n}_{\text{eq}}/\text{cm}^2$ , <10kGy
    - Organic scintillator/steel possible in the barrel HCAL (R&D on radiation tolerance) → read-out by SiPMs or wavelength shifting fibres + SiPMs
    - Many other existing technologies would also be applicable
- **Possible technologies – R&D needs**
  - **Noble liquid calorimetry:** Development of highly granular read-out electrodes and low-noise read-out, high-density signal feedthroughs, low-material cryostats (composite or Al-alloy honeycomb) (TF6)
  - **Scintillator based calorimetry:** Radiation hardness of scintillators and SiPMs (TF4). R&D on radiation hard inorganic scintillators, crystal fibres (SPACAL type)
  - **Si-based calorimetry:** Radiation hardness, cost- and material reduction through monolithic designs with integrated sensor and readout (TF3)
  - **For all technologies:** Timing resolution at the O(25ps) level or better would help to reduce pile-up

# Challenges for Calorimetry – R&D Needs (TF6)

- **High granularity** (lateral cell sizes of  $\leq 2\text{cm}$ , like for the proposed reference detector LAr calorimeter)
  - Particle flow (measure each particle where it can be best measured)
  - 5D calorimetry (imaging calorimetry, including timing)  $\rightarrow$  use of MVA based reconstruction (Neural Networks, ...)
  - Pile-up rejection
    - Efficient combined reconstruction together with the tracker
- **Timing for pile-up rejection, 5D calorimetry:**
  - $O(25\text{ps})$  to reduce pile-up by factor 5 ( $\langle\mu\rangle = 1000 \rightarrow 200$ )  $\rightarrow$  LGADs, 3D pixel sensors  $\rightarrow$  further R&D on pad sizes and radiation hardness
  - $O(5\text{ps})$  to reduce pile-up by factor 25 ( $\langle\mu\rangle = 1000 \rightarrow 40$ )  $\rightarrow$  ultra-fast inorganic scintillators, ultra-thin LGADs
- **Data rates – Triggering**
  - Noble-liquid calorimetry + scintillator/Fe HCAL:  $O(3\text{M})$  channels 200 – 300TB/s  $\rightarrow$  full read-out at 40MHz (like ATLAS in HL-LHC)
  - Si option: many more channels, zero suppression on-detector necessary
  - $\rightarrow$  100Gbps data links, off-detector real-time event processing with advanced hardware (GPUs, FPGAs)
  - $\rightarrow$  on-detector processing with radiation tolerant processing
- **Crazy ideas for the future:** Possible “maximal information” calorimeter: divided into small detection volumes (voxels) that measure ionization, time, and Cherenkov and scintillation light simultaneously
  - e.g. noble liquid calorimetry

# Gaseous detectors

For 100 TeV proton colliders, gas-based detectors considered only feasible for muon system, calorimeter.

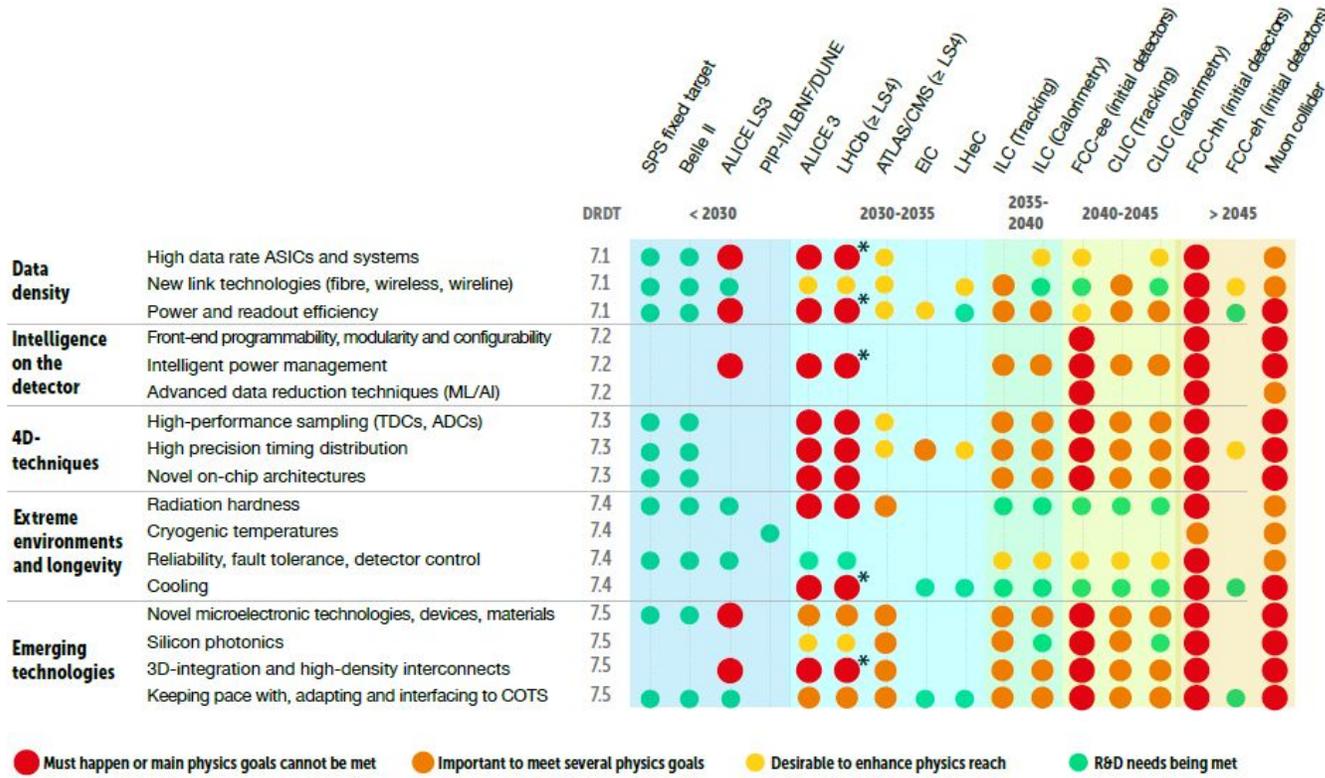


high priority items are:

- radiation hardness
- fine granularity
- eco-gas
- rate capability

Synergies: for muon detectors, unique rad-hardness and rate capability requirements. For calorimeters, good overlap with muon collider, except in rate capability.

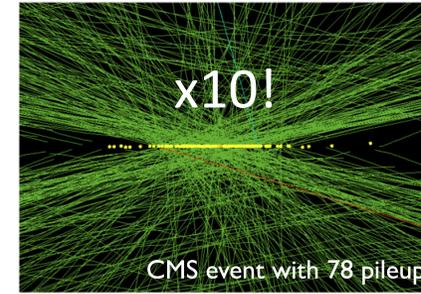
# electronics and data processing



For 100 TeV proton colliders, much R&D needed. Basically everything is not up to its demands. Tracker readout with zero suppression is 800 TB/s. Industry is improving rate, but doesn't care about rad hardness. May need photonics integrated into CMOS.

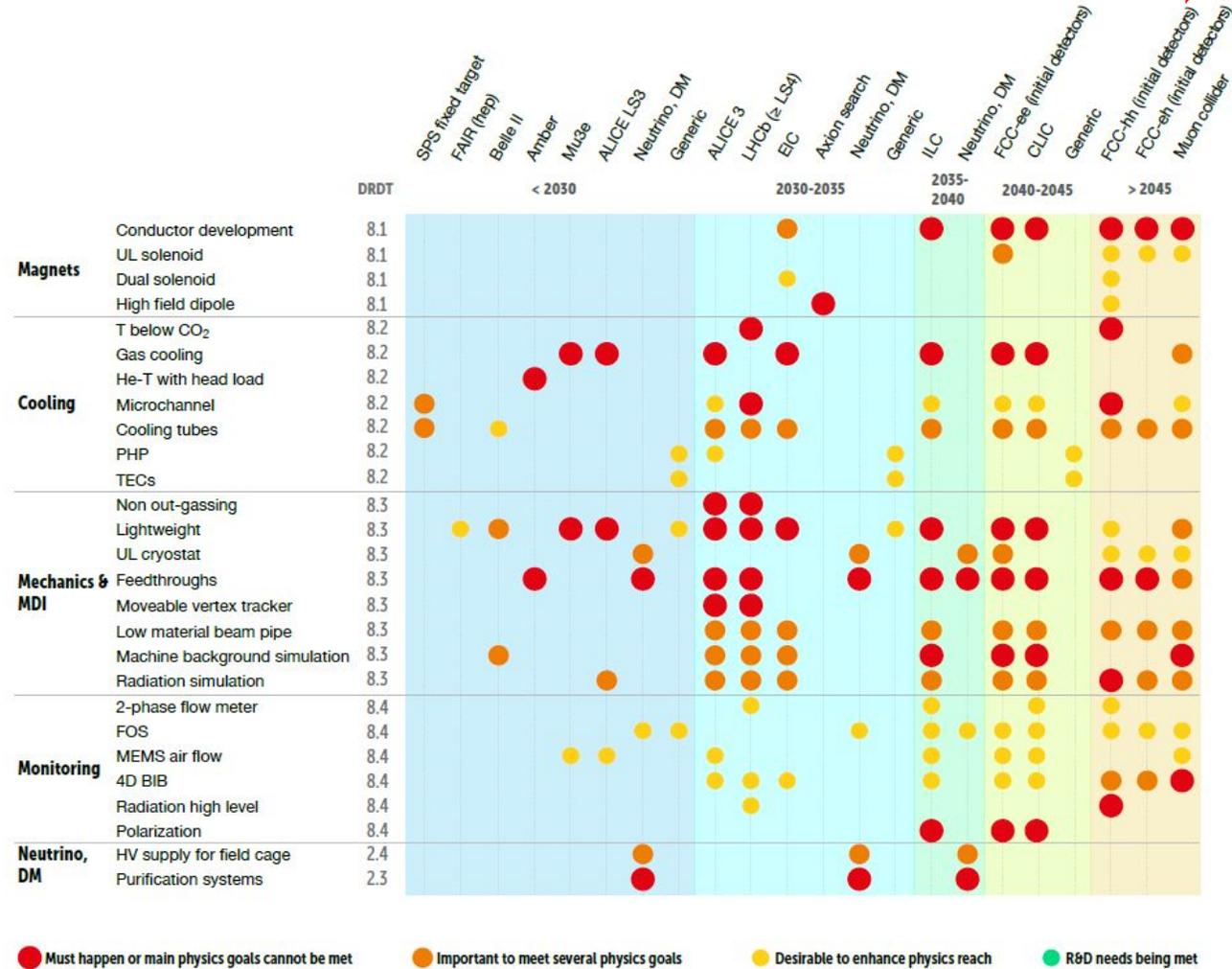
Synergies: many with muon collider, some with FCC-ee

# Software and Computing



- FCC-hh will be an unprecedented challenge for software & computing
- **Pile up** is foreseen to be potentially up to 1000 interactions per bunch crossing (HL-LHC: 200)
  - New approaches needed for reconstruction algorithms, particularly tracking (machine learning?)
  - Integrated design of detectors and software needed to mitigate pile up
- **Gigantic data volumes** (luminosity, detector, ... ) will also pose challenges for computing infrastructure due to large data volume: storage, networking, end-user analysis, etc.
- Possibility of completely different software & computing hardware **paradigms** on such a timescale
  - Quantum computing?
  - Real-time analysis?
  - ...
- Exploiting **common software** with FCC-ee: [FCCSW](#) using [KEY4HEP](#) → Not restricted to FCC, common software needs to be more broadly applied

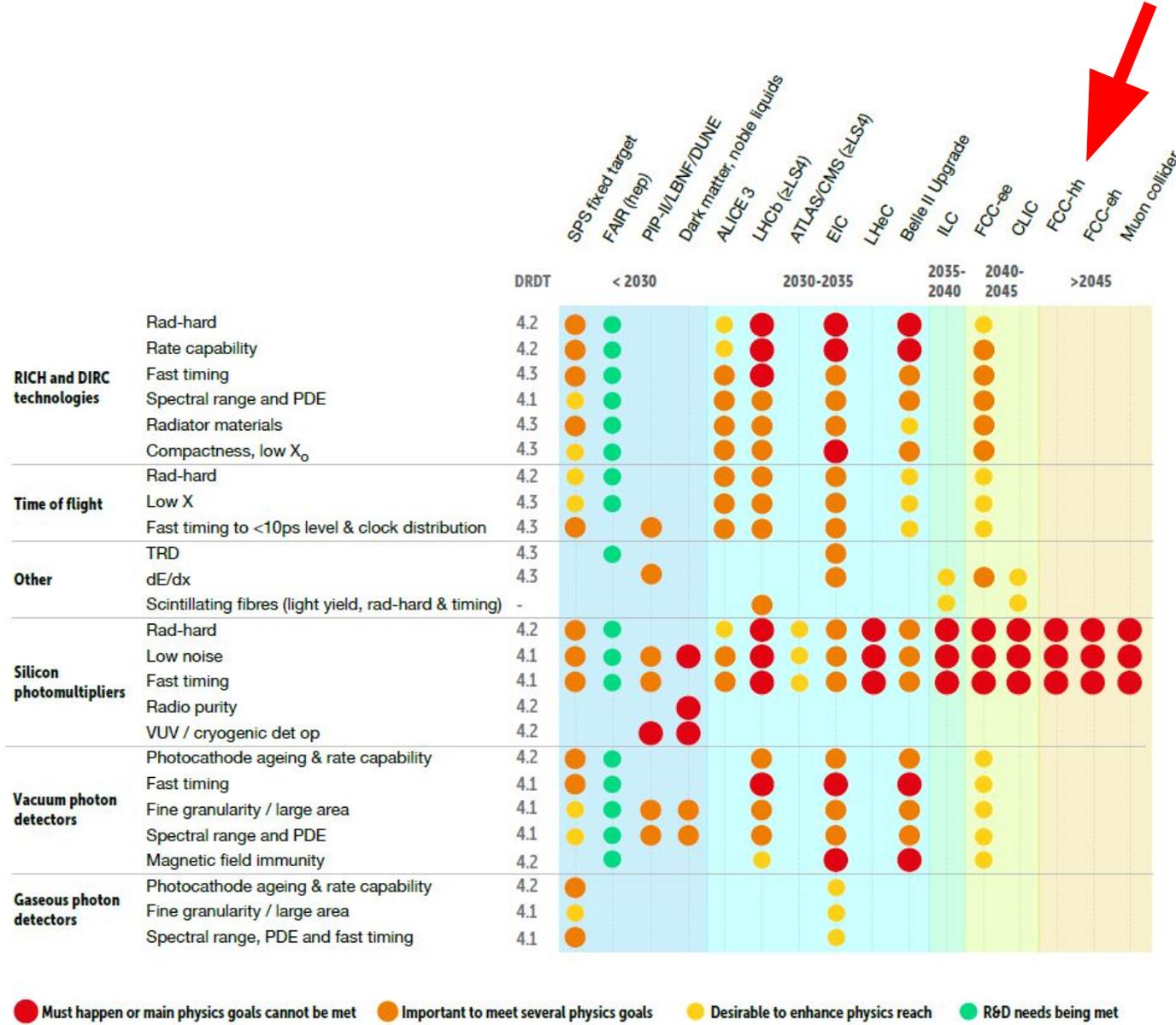
# Integration



For 100 TeV proton colliders, high priority items are:

- magnets
- cooling systems
- improved radiation simulations
- dense signal feedthroughs, especially for Liquid Argon

# particle identification and photons



For 100 TeV proton colliders, work is on photodetectors

high priority items are:

- rad hard
- low noise
- fast timing

Synergies: strong synergies with FCC-ee and muon collider

Suspect we may find later that FCC-hh would benefit from TOF, TRD, dE/dx, maybe even RICH

● Must happen or main physics goals cannot be met ● Important to meet several physics goals ● Desirable to enhance physics reach ● R&D needs being met

# specialized detectors

In addition, understanding any specialized detectors for forward physics or long-lived particles needs to be done early, as this affects cavern design.

# Conclusions

- In order to take advantage of the great physics potential of FCC-hh, extensive detector development is necessary.
- the exact needs are documented in the FCChh CDR, and the BRN and ECFA roadmap

# Backup

# FCC-hh parameters

**Table 2.1.** FCC-hh baseline parameters compared to the LHC and HL-LHC parameters.

	LHC	HL-LHC	FCC-hh	
			Initial	Nominal
<b>Main parameters and geometrical aspects</b>				
c.m. Energy (TeV)	14		100	
Circumference C (km)	26.7		97.75	
Dipole field (T)	8.33		<16	
Arc filling factor	0.79		0.8	
Straight sections	8 × 528 m		6 × 1400 m + 2 × 2800 m	
Number of IPs	2 + 2		2 + 2	
Injection energy (TeV)	0.45		3.3	
<b>Physics performance and beam parameters</b>				
Peak luminosity <sup>1</sup> (10 <sup>34</sup> cm <sup>-2</sup> s <sup>-1</sup> )	1.0	5.0	5.0	<30.0
Optimum average integrated lumi/day (fb <sup>-1</sup> )	0.47	2.8	2.2	8
Assumed turnaround time (h)			5	4
Target turnaround time (h)			2	2
Peak no. of inelastic events/crossing	27	135 (lev)	171	1026
Total/inelastic cross section $\sigma$ proton (mbarn)		111/85		153/108
Luminous region RMS length (cm)			5.7	5.7
Distance IP to first quadrupole, L* (m)		23	40	40
<b>Beam parameters</b>				
Number of bunches $n$		2808	10 400	
Bunch spacing (ns)	25	25	25	
Bunch population $N(10^{11})$	1.15	2.2	1.0	
Nominal transverse normalised emittance ( $\mu\text{m}$ )	3.75	2.5	2.2	2.2
Number of IPs contributing to $\Delta Q$	3	2	2+2	2
Maximum total b-b tune shift $\Delta Q$	0.01	0.015	0.011	0.03
Beam current (A)	0.584	1.12	0.5	
RMS bunch length <sup>2</sup> (cm)		7.55	8	
IP beta function (m)	0.55	0.15 (min)	1.1	0.3
RMS IP spot size ( $\mu\text{m}$ )	16.7	7.1 (min)	6.8	3.5
Full crossing angle ( $\mu\text{rad}$ )	285	590	104	200 <sup>3</sup>
<b>Other beam and machine parameters</b>				
Stored energy per beam (GJ)	0.392	0.694	8.3	
SR power per ring (MW)	0.0036	0.0073	2.4	
Arc SR heat load (W/m/aperture)	0.17	0.33	29	
Energy loss per turn (MeV)		0.0067	4.67	
Critical photon energy (keV)		0.044	4.3	
Longitudinal emittance damping time (h)		12.9	0.5	
Transverse emittance damping time (h)		25.8	1.0	
Dipole coil aperture (mm)		56	50	
Minimum arc beam half aperture (mm)		~18	13	
Installed RF voltage (400.79 MHz) (MV)		16	48	
Harmonic number		35 640	130 680	

**Notes.** <sup>1</sup>For the nominal parameters, the peak luminosity is reached during the run. <sup>2</sup>The HL-LHC assumes a different longitudinal distribution; the equivalent Gaussian is 9 cm. <sup>3</sup>The crossing angle will be compensated using the crab crossing scheme.

# Advances in SC Magnets for Accelerators

## Past:

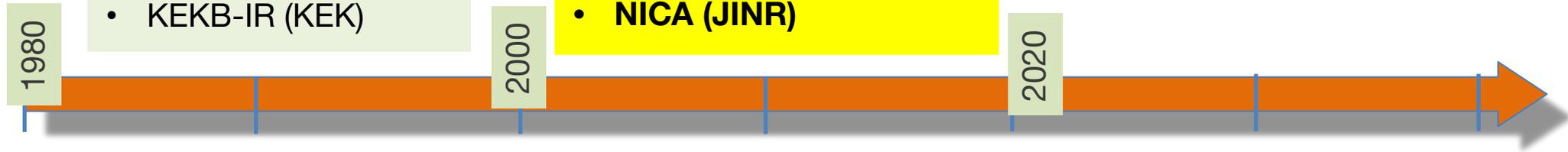
- ISR-IR
- Tevatron (Fermilab)
- TRISTAN-IR (KEK)
- HERA (DESY)
- Nuclotron (JINR)
- LEP-IR (CERN)
- KEKB-IR (KEK)

## Present:

- RHIC (BNL)
- LHC (CERN)
- SRC (RIKEN) ..... *SC-Cyclotron*
- Under Construction
- FAIR (GSI) ..... *Fast-cycle Shnchr.*
- **HL-LHC (CERN)**
- NICA (JINR)

## Future:

- EIC (e-Ion)
- FCC-hh / HE-LHC
- SppC



Tevatron-D.

HERA-D.

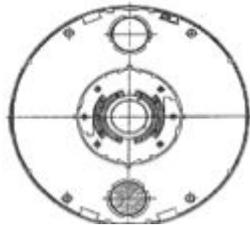
RHIC-D.

LHC-D (NbTi)

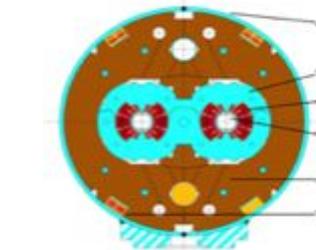
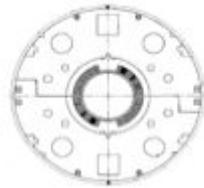
HL-LHC 11T-D ( $Nb_3Sn$ )



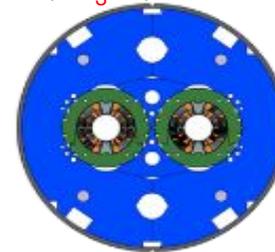
ISR-IRQ, LEP-IRQ



TRISTAN/KEKB-IRQ

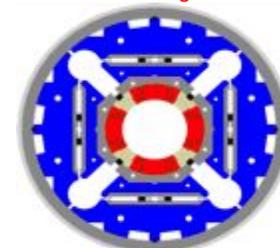
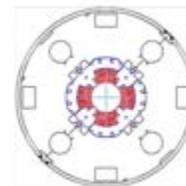
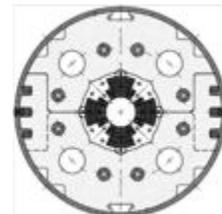
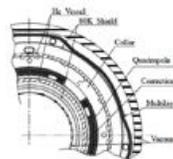
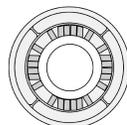
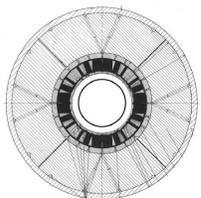


LHCC-IRQ (NbTi)



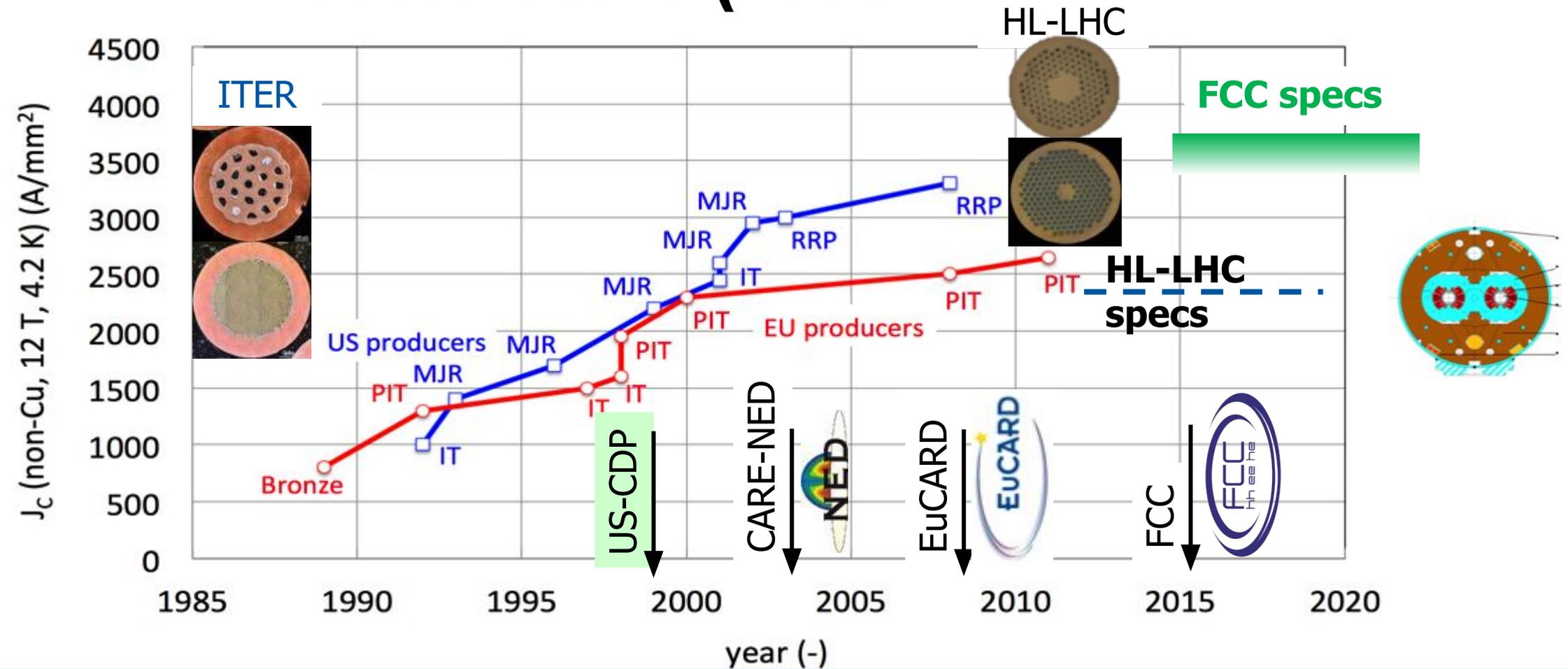
HL-LHC-IRQ ( $Nb_3Sn$ )

Dipole



IR Quadrupole

# Nb<sub>3</sub>Sn Conductor development for Accelerators (1998 ~ )



After 10 years of development, the US and EU development gave us the Nb<sub>3</sub>Sn conductor for HILUMI.

# Personal View on Relative Timelines

Timeline	~ 5	~ 10	~ 15	~ 20	~ 25	~ 30	~ 35
<b>Lepton Colliders</b>							
SRF-LC/CC	Proto/pre-series	Construction		Operation		Upgrade	
NRF-LC	Proto/pre-series	Construction		Operation		Upgrade	
<b>Hadron Collider (CC)</b>							
8~(11)T NbTi/(Nb <sub>3</sub> Sn)	Proto/pre-series	Construction		Operation			Upgrade
12~14T Nb <sub>3</sub> Sn	Short-model R&D	Proto/Pre-series		Construction		Operation	
14~16T Nb <sub>3</sub> Sn	Short-model R&D		Prototype/Pre-series			Construction	

**Note:** LHC experience: NbTi (10 T) R&D started in 1980's --> (8.3 T) Production started in late 1990's, in ~ 15 years

# High-Field Magnet Development for HEP in Europe – A Roadmap by the LDG

## HFM Expert Panel, 2022

LDG Expert Panel: [B. Auchmann](#) (PSI/CERN), A. Ballarino (CERN), B. Baudouy (CEA Saclay), L. Bottura (CERN, *Technical Secretary*), P. Fazilleau (CEA Saclay), L. Garcia-Tabarés (CIEMAT, *Co-Chair*), M. Noe (KIT), S. Prestemon (LBNL), E. Rochepault (CEA Saclay), L. Rossi (INFN Milano), B. Shepherd (STFC), C. Senatore (Uni Geneva), P. Védérine (CEA Saclay, *Chair*) HFM Project at CERN: A. Siemko (CERN); PSI MagDev Team: D. M. Araujo, A. Brem, M. Daly, M. Duda, C. Hug, J. Kosse, T. Michlmayr, H. G. Rodrigues, S. Sanfilippo

B. Auchmann, ICHEP 2022

## HFM R&D Goals

1. Demonstrate **Nb<sub>3</sub>Sn magnet technology** for large scale deployment, pushing it to its limits in terms of maximum field and production scale.
  - a. The effort to quantify and demonstrate Nb<sub>3</sub>Sn ultimate field comprises the development of conductor and magnet technology towards the ultimate Nb<sub>3</sub>Sn performance.
  - b. Develop Nb<sub>3</sub>Sn magnet technology for collider-scale production, through robust design, industrial manufacturing and cost reduction.
2. Demonstrate the **suitability of HTS for accelerator magnets**, providing a proof-of-principle of HTS magnet technology beyond the reach of Nb<sub>3</sub>Sn.

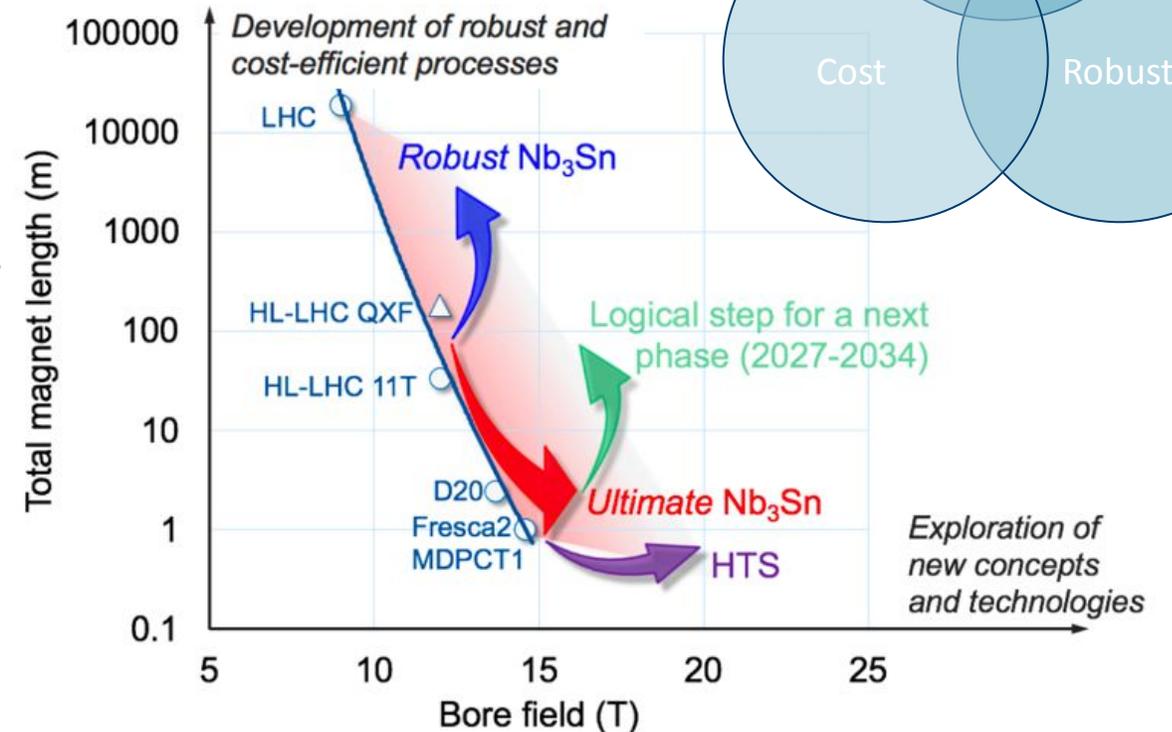
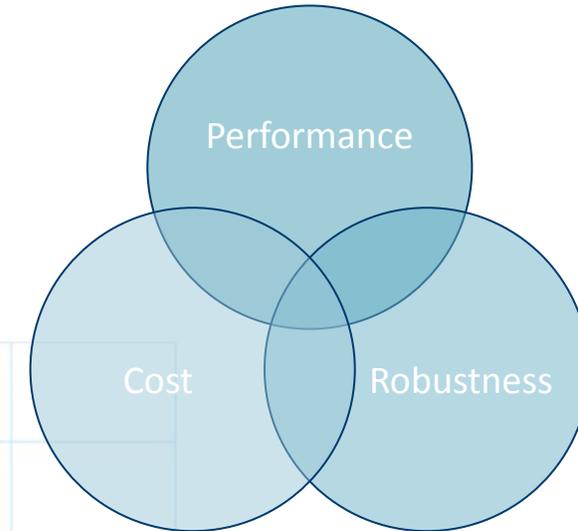


Fig. 2.7: Graphical representation of the objectives of the HFM R&D programme from 2021–2027. Both fronts of maximum field (red for Nb<sub>3</sub>Sn, purple for HTS) and large-scale production (blue) will be advanced. Also represented, in green, is a possible evolution for the longer term, 2027–2034.

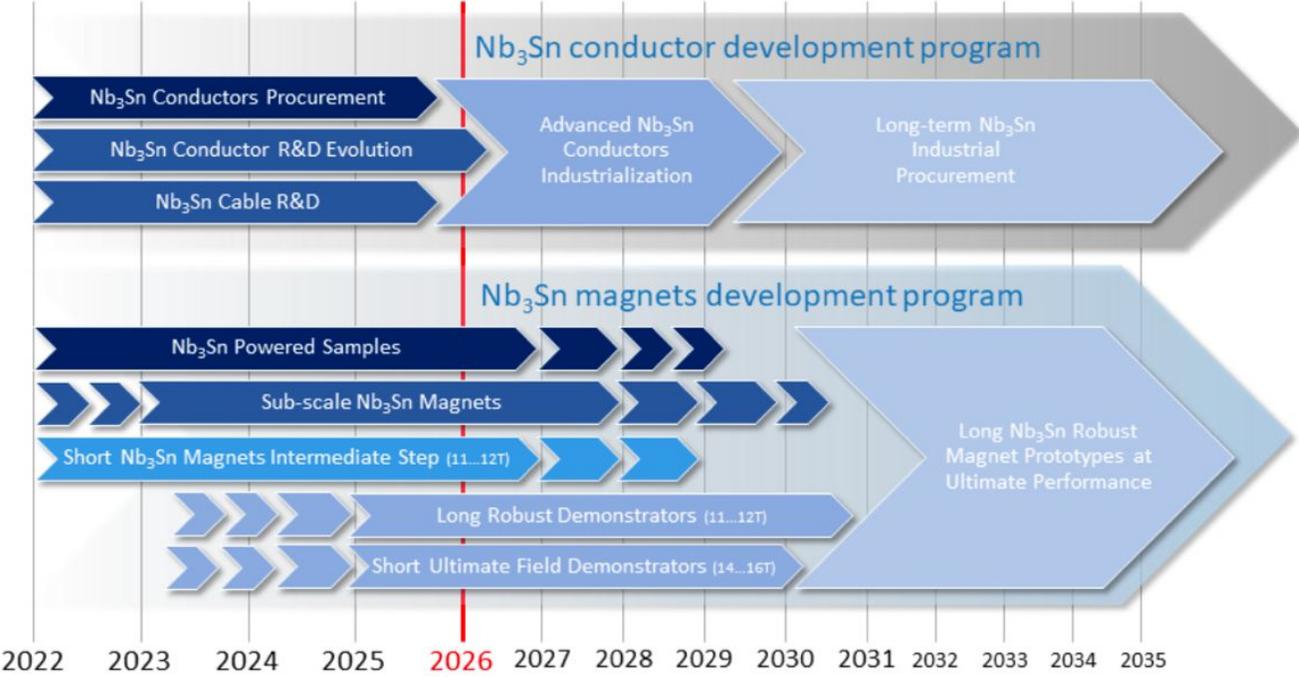
# High-Field Magnet Development for HEP in Europe – A Roadmap by the LDG

## HFM Expert Panel, 2022

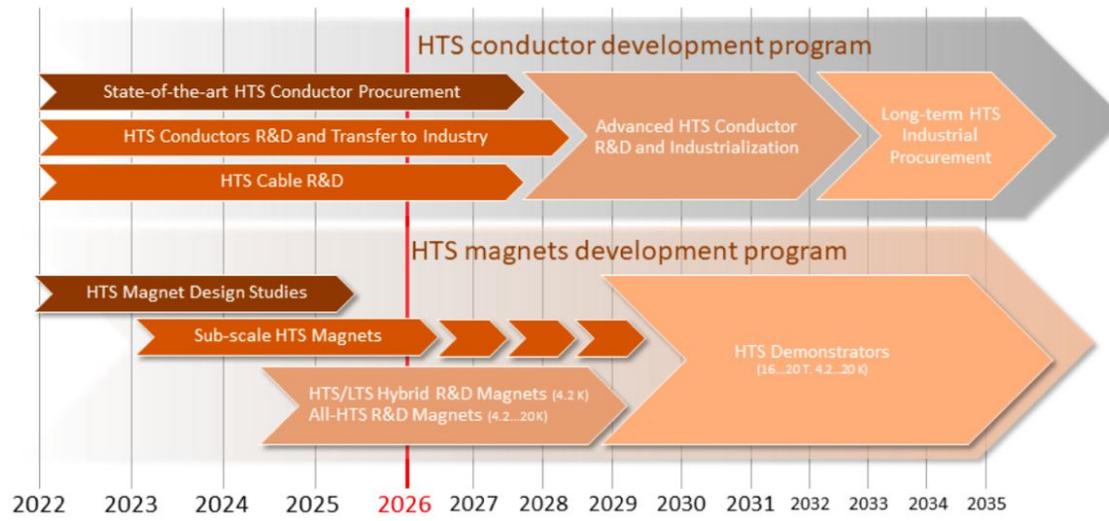
B. Auchmann, ICHEP 2022

LDG Expert Panel: B. Auchmann (PSI/CERN), A. Ballarino (CERN), B. Baudouy (CEA Saclay), L. Bottura (CERN, *Technical Secretary*), P. Fazilleau (CEA Saclay), L. Garcia-Tabarés (CIEMAT, *Co-Chair*), M. Noe (KIT), S. Prestemon (LBNL), E. Rochepault (CEA Saclay), L. Rossi (INFN Milano), B. Shepherd (STFC), C. Senatore (Uni Geneva), P. Védrine (CEA Saclay, *Chair*)  
 HFM Project at CERN: A. Siemko (CERN); PSI MagDev Team: D. M. Araujo, A. Brem, M. Daly, M. Duda, C. Hug, J. Kosse, T. Michlmayr, H. G. Rodrigues, S. Sanfilippo

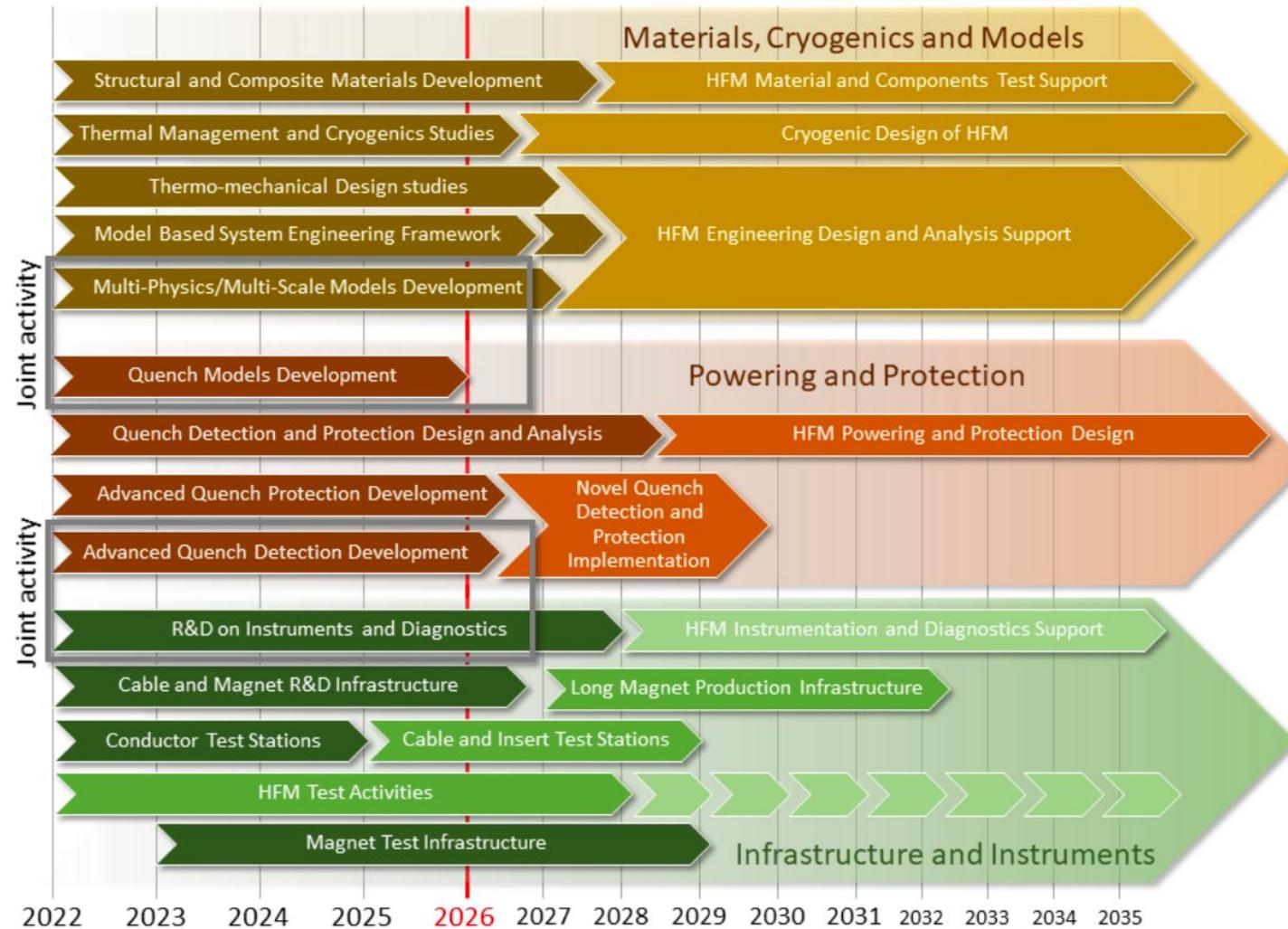
### HFM R&D Timeline – Nb<sub>3</sub>Sn Conductor and Magnet R&D



### HFM R&D Timeline – HTS Conductor and Magnet R&D

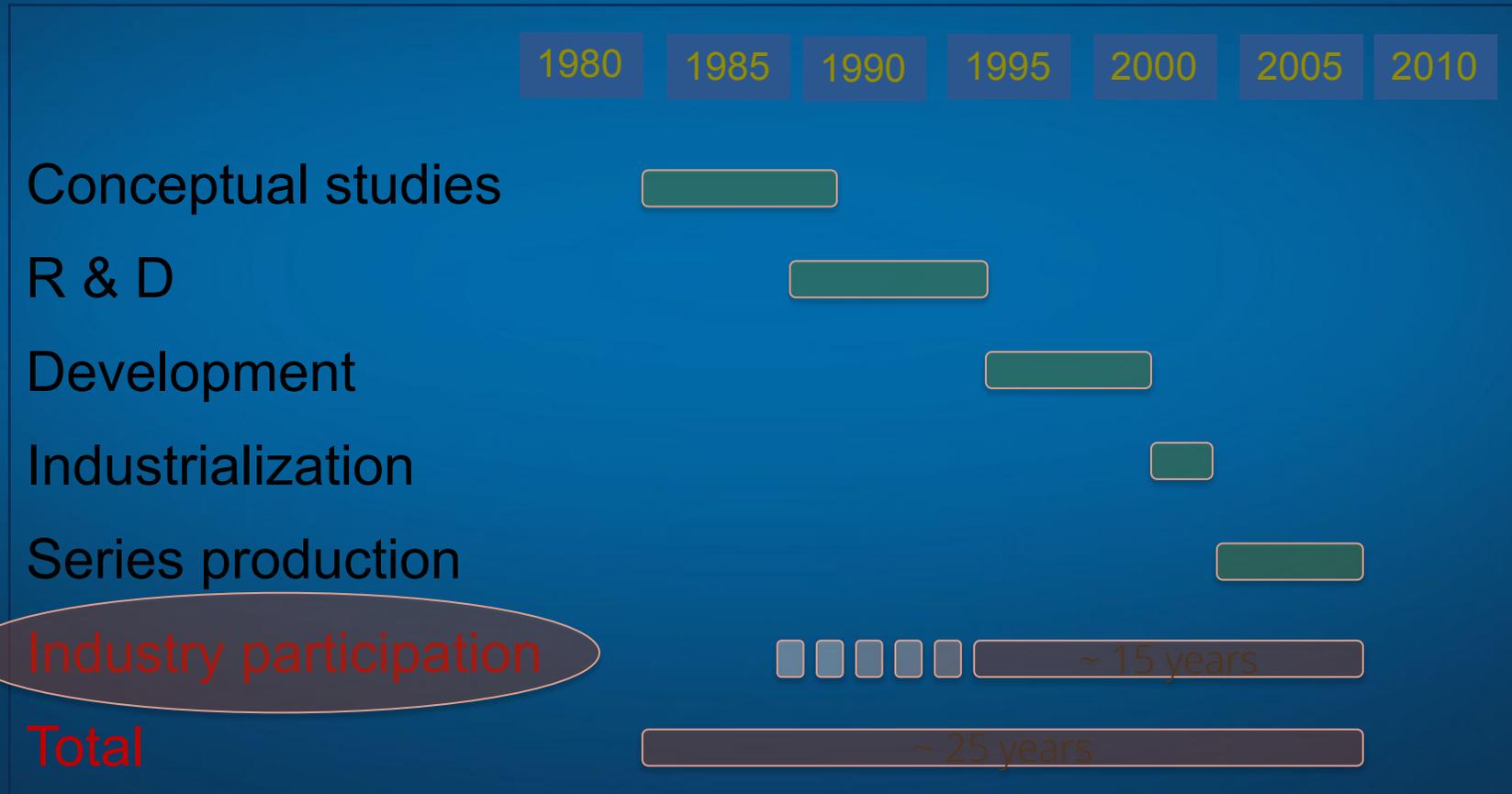


# HFM R&D Timeline – Cross-Cutting R&D Activities



# Time Indicator

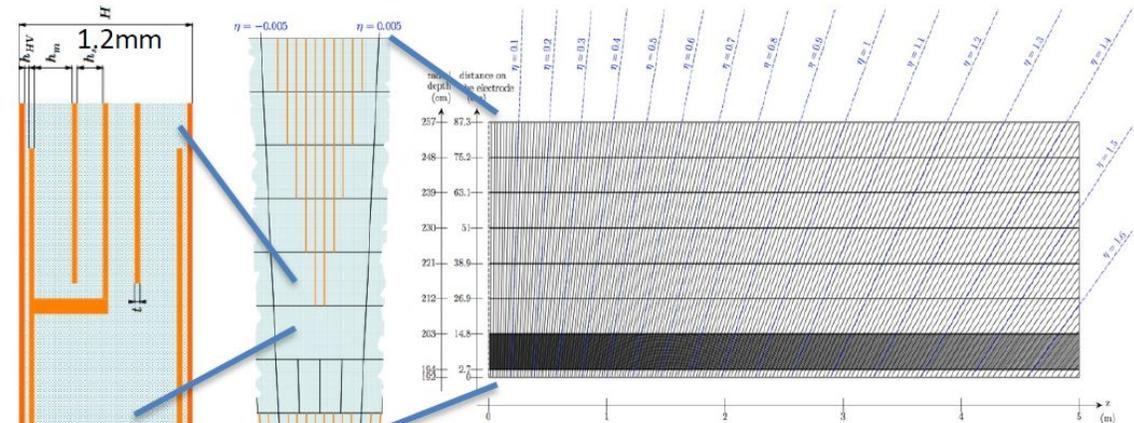
Case: LHC superconducting dipole magnets



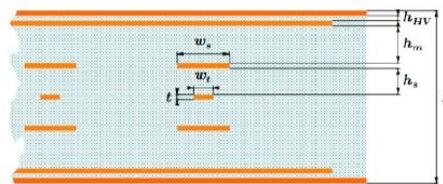
# LAr Calorimeter: How to Achieve High Granularity?

## Realize electrodes as multi-layer PCBs (1.2mm thick), 7 layers

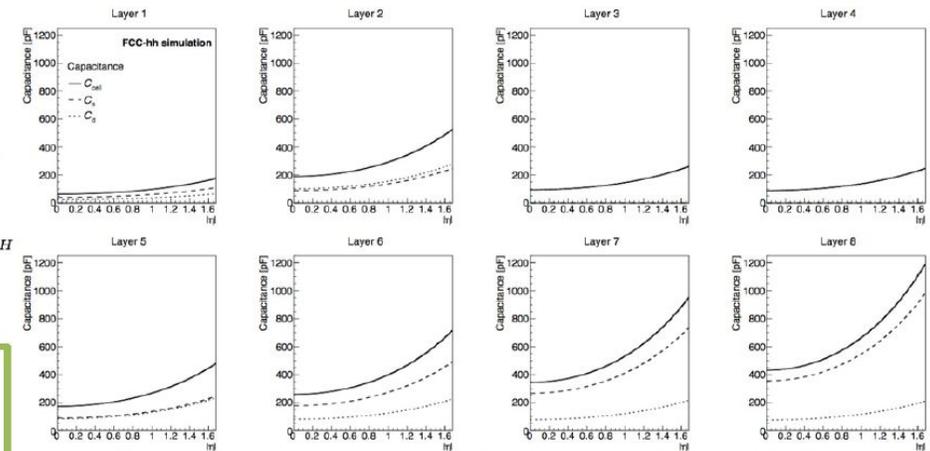
- HV and read-out
- Signal traces (width  $w_t$ ) in dedicated signal layer connected with vias to the signal pads
- Traces shielded by ground-shields (width  $w_s$ ) forming  $25\Omega - 50\Omega$  transmission lines
- $\rightarrow$  capacitance between shields and signal pads  $C_s$  will add to the detector capacitance via the gap  $C_d$
- $\rightarrow C_{cell} = C_s + C_d \approx 25 - 1000\text{pF}$
- The higher the granularity the more shields are necessary  $\rightarrow C_s$  increases,  $C_d$  decreases (smaller cells)



- $\rightarrow$  Serial noise contribution proportional to capacitance  $C_{cell}$
- $\rightarrow$  0.5 – 40MeV noise per read-out channel assuming ATLAS-like electronics
- $\rightarrow$   $\leq 0.1$  MeV possible with cold preamplifiers



**Hadronic showers:**  
Energy sums over  $O(500-10000)$  cells



Plots A. Zaborowska, J. Faltova

## 21 Will FCC-ee delay FCC-hh?

**The FCC-ee will not delay FCC-hh: instead, it will make it a realizable dream, and will maximize the significance of its physics results.** One of the great benefits of the choice of FCC-ee (with respect to any other form of lepton collider) is that it allows high-level technological effort to be concentrated on high-field magnets, while the technically simpler precursor of FCC-hh is built and operated. What appears to be additional time can be used to investigate newer, more ambitious technologies, with the possible result of a more affordable 100 TeV collider, or even a higher-energy collider (150 TeV or more?). There is no doubt that the 15 years foreseen for FCC-ee operation will be put to good use in this perspective.

Moreover, the sequential implementation (first FCC-ee, then FCC-hh) maximizes the physics reach at the precision and the energy frontiers far beyond that of any linear collider project, taking advantage of the multiple complementarities and synergies of FCC-ee and FCC-hh [2]. Finally, paying for FCC-hh within the timescale proposed for FCC-ee would require a substantial budgetary boost.

Other investigations for the high-energy frontier, such as the muon collider or plasma wake-field acceleration, could be pursued in parallel as accelerator R&D projects.

## 22 How long will the Shutdown between FCC-ee and FCC-hh be?

The schedule of the FCC integrated programme foresees 15 years of FCC-ee operation and 25 years of FCC-hh operation, interleaved with a shutdown of 10 years to dismantle the lepton collider and install the hadron collider in the FCC tunnel. This estimate for the shutdown duration results from an in-depth study based on past experience at CERN and on the planning optimization for civil engineering and infrastructure realization. It has been argued, however, that a simple extrapolation of the LEP-LHC transition to the transition from FCC-ee to FCC-hh could lead to a much longer duration [104].

A brief account of the LEP-LHC transition period can be found in Ref. [105]. The Large Electron Positron collider was shut down on 2 November 2000, to make way for the installation of the Large Hadron Collider in the same tunnel [106], with an envisaged transition time of about four years. The LEP dismantling [107] started on 27 November 2000 and, after three months, the most critical two-thirds of the LEP ring had been emptied [105]. Surveying for the LHC started in November 2001 in the empty LEP tunnel [108], so LEP dismantling took less than a year before work for the LHC could start. The last piece of LEP went to the surface in February 2002 [109]: **The LEP dismantling caused no delay in the LHC installation. This experience gives no reason to believe that the FCC-ee dismantling will cause any delay to the FCC-hh installation.**

1. Significant infrastructure work was needed for the LHC, in particular the excavation of the new, large, caverns for ATLAS and CMS;
2. A financial crisis – possibly caused by an underestimation of the LHC cost – arose, leading to a redefinition of the cost to completion and of the commissioning schedule [110], and delaying in turn the start of LHC to 2007;
3. The mass production of the LHC dipole cold masses was handed over to industry [111] in December 2001 (i.e., after the end of LEP dismantling), and the tender was concluded in spring 2002. By December 2003, CERN had taken delivery of 154 LHC dipoles (out of a total of 1232), on which a considerable amount of testing was still necessary [112].

The installation of the cryogenic line (QRL) started in August 2003 and, after many difficulties [113], was complete in November 2006. The first magnet was lowered in the tunnel on 7 March 2005 [114], **the full installation of the accelerator was completed in spring 2008, and the first circulating beam in the LHC was celebrated on 10 September 2008 [115], i.e. within three and a half years after the beginning of the magnet installation.** A major incident took place only three weeks later when a faulty electrical connection between two dipole magnets opened, triggering an electrical arc that punctured the helium vessel. The resulting high pressure helium gas wave damaged a few hundred meters of beam line, and caused the loss of approximately six tonnes of liquid helium. This incident was quickly analysed and a repair plan designed [116]. This delayed the first beam in LHC as well as first collisions to the end of 2009 [117], and the real start of physics to early 2010.

The conclusion drawn from this analysis of the LEP-LHC shutdown can be summarized as follows.

- As discussed in Section 23.1, gaining approval for the LHC was greatly facilitated by the existence of the LEP tunnel;
- The installation of the LHC in the LEP tunnel did not slow down the completion of LHC, but rather made it easier compared to having to excavate and complete a new infrastructure. The LEP dismantling took less than a year. Although the LEP tunnel was initially not designed to host a 14 TeV hadron collider, the installation of the LHC accelerator itself, thanks to extraordinary efforts, was quite rapid, about three years. **A transition period of 10 years for the FCC is therefore quite a reasonable evaluation;**
- The LHC delays during this period were largely intrinsic to the readiness of LHC itself, which was still in a preparatory phase when the LEP dismantling was over. A corollary message for the FCC-hh installation, is that **the best way to ensure a short transition between two machines is to make sure that the the second one is ready to install before the first machine is shut down;**

The FCC schedule is prepared in such a way as to avoid the planning- and infrastructure-related issues that made the LHC installation difficult. In particular: the tunnel diameter is much larger (5.5 m instead of 3.8 m), enabling easier installation; the large experimental caverns are to be built at the beginning of the project already for FCC-ee; the dipole magnets are being studied already today, so that mass production can start well before the initiation of FCC-hh installation; finally, FCC-ee will not be pushed to its absolute limit in the hope of finding a new particle in the last year: the transfer of scientific personnel from one FCC to the other should be much smoother.

**The planned 10-year period for the FCC-ee to FCC-hh transition takes into account the lessons learned from the LEP-LHC transition. This schedule estimate is technically solid and not aggressive.**

## 23.3 Should we by-pass FCC-ee and go directly for a 100 or 150 TeV Hadron Collider?

Given the cost of 24 GCHF for the FCC-hh in a standalone scenario, and given the status of high-field magnet R&D and the anticipated target cost of the magnets, it is not a realistic scenario to expect that such a machine could start operation in the 2040's. A further disadvantage is that the use of the infrastructure would be reduced to one project, thus increasing its cost per year of foreseeable use. The opportunity to build the FCC-ee and to profit from its impressive and largely unique exploratory programme would be lost, and the physics output of the FCC-hh would be significantly diminished.

The clear case for an  $e^+e^-$  collider would have to be satisfied elsewhere in the world. If it is a linear collider, it seems that this could not happen without a large European contribution, further limiting the ability of CERN to invest in its own infrastructure. If it were instead a circular collider in China, the likelihood that it would be followed by a hadron collider is high. In that case, CERN might miss the opportunity to build this powerful exploration machine. (The case of a circular  $e^+e^-$  collider in China is discussed more completely in Section 23.7.)

# FCC-ee versus FCC-hh

<https://arxiv.org/abs/1906.02693>

## 23.4 Should we by-pass FCC-ee and opt for a High-energy Upgrade of the LHC instead?

This scenario (HE-LHC) is part of the FCC study, and is the object of its 4th CDR and ESPP contribution [118, 119]. It would consist of replacing the LHC dipoles with 16 T magnets to reach a centre-of-mass energy of 27 TeV, doubling the mass reach for BSM searches and providing interesting prospects for Higgs physics, particularly for  $t\bar{t}H$  and  $HH$  production. The HE-LHC option could also be interesting if the HL-LHC discovered new physics at the high end of its mass reach. Doubling the LHC energy would indeed increase the production rates of the new phenomena, and allow a more detailed study. The current strong limits already obtained by the LHC, however, make even the doubling of energy a rather limited step to fully explore potential scenarios for new physics, of which a LHC discovery could just be the lowest-lying state of the spectrum. An accurate judgement of the added value of HE-LHC following a LHC discovery would therefore depend on its specific features.

The HE-LHC has long been seen as an appealing scenario, in the absence of a realistic evaluation of its cost and inherent difficulties. The HE-LHC would be extremely constrained by the small diameter of the LHC tunnel, 3.8 m instead of the 5.5 m diameter foreseen for the FCC tunnel. The injection energy from the SPS forces the magnets to have the same physical aperture as those of the LHC, thus increasing their cost in comparison with those that could be used in FCC-hh. An alternative possibility would be to rebuild the SPS to a higher energy machine, adding to the expense.

The HE-LHC project cost has been estimated to 7 GCHF, i.e. 3 GCHF more than the marginal cost of FCC-ee, with a minimal fraction of the infrastructure reusable for the FCC-hh. Additionally, the installation of HE-LHC would occupy the LHC infrastructure for at least 6 years after the end of HL-LHC, without collider physics at CERN. Adding an estimated 20 years of operation, followed by the adaptation of the machine to serve as an injector for FCC-hh, this would take us well into the 2060's before the 100 TeV machine could start, which is not earlier than in the integrated FCC scenario.

**En route to 100 TeV, the physics return that can be guaranteed by the HE-LHC is smaller than that of the FCC-ee option. Direct discoveries are clearly possible, and could modify our assessment, but otherwise the added value with respect to the HL-LHC appears inferior to the greatly complementary inputs provided to FCC-hh by the FCC-ee physics programme. The benefits of the HE-LHC path to 100 TeV are further reduced by the additional costs, relative to the integrated FCC plan (ee+hh).**

# FCC-ee versus others

<https://arxiv.org/abs/1906.02693>

## 23.5 Rather than starting with FCC-ee, should we build a Lower-Energy Hadron Collider in the FCC Tunnel?

This possibility, which is not part of the FCC design study, has been raised during discussions at the European Strategy symposium in Granada, and subsequently. It involves staging FCC-hh by using, in a first iteration of the project, magnets based on established NbTi technology [120]. We shall refer to this phase as LE-FCC (low-energy FCC). This approach is supported by two considerations: (i) a tested technology puts magnet construction on a fast track, bypassing the 20-year long, challenging R&D phase foreseen by the FCC-hh CDR; and (ii) the cost of this first stage would be lower than the full 100 TeV collider. Studies have begun to evaluate the cost of the project, in relation to various performance scenarios for energy and luminosity. It already appears quite clear that even an LHC-like magnet design, which would allow an energy of about 48 TeV to be reached, would lead to a cost far exceeding the cost of the FCC-ee accelerator, and beyond the budget targets that could allow operation by 2043. Important savings can be obtained by reducing the magnetic field below 6 T, leading to an energy of 37.5 TeV or less. Aside from the physics considerations, the assessment of the outcome of these studies should weigh the additional cost relative to the FCC-ee phase, cost that would increase the overall budget required to attain the ultimate target of 100 TeV, putting in jeopardy the upgrade of the first stage to ultimate performance.

As we wait for the technical assessment, we focus here on some general physics considerations. The physics case of FCC-hh builds on three pillars: (i) its contribution to the Higgs, EW and top precision measurement programme; (ii) its direct discovery reach at high mass; and (iii) its potential to conclusively answer questions like whether dark matter (DM) is a thermally-produced weakly-interacting massive particle (WIMP), or whether the electroweak phase transition was of strong first order or not. **For all three sets of goals, the choice of 100 TeV as target energy plays an essential role, which, contrary to statements occasionally heard, is fully justified and required,** as briefly summarized here.

The precision Higgs measurements of FCC-hh, documented in the Physics CDR [2], benefit in several ways from high energy. On one hand, higher energy leads to larger inclusive statistics. On the other, the extended kinematic reach enables measurements with reduced systematic uncertainty, and probes Higgs interactions at large  $Q^2$ , where the sensitivity to deviations from

the SM is enhanced and complementary to that at a low-energy  $e^+e^-$  Higgs factory. The precise measurement of ratios of branching ratios such as  $B(H \rightarrow X)/B(H \rightarrow 4\ell)$  ( $X = \gamma\gamma, \mu^+\mu^-, Z\gamma$ ) will likely reach the level of per-cent precision even at  $\sqrt{s} \sim 40$  TeV, with  $L \sim 10\text{ab}^{-1}$ . But the measurement of the Higgs self-coupling will be significantly penalized, since the total rate at  $\sqrt{s} = 37.5$  TeV (48 TeV) would be smaller by a factor of 5 (3), with respect to 100 TeV. The potentially precise determination of the ttH coupling, from the ratio of ttH/ttZ with highly boosted final states, would likewise suffer from statistics (and from the uncertain knowledge of the ttZ EW coupling, in absence of a dedicated measurement above the  $e^+e^- \rightarrow t\bar{t}$  threshold at FCC-ee). The resulting uncertainty on the ttH coupling would enter as dominant uncertainty in the extraction of the Higgs self-coupling from the measurement of the  $gg \rightarrow \text{HH}$  production rate, where the ttH coupling plays a key role in the cancellation between (triangle) self-coupling and (box) double emission diagrams. All things considered, the measurement of the Higgs self-coupling at LE-FCC would have an uncertainty at least two to three times larger than FCC-hh: 100 TeV and  $30\text{ab}^{-1}$  are the minimum requirement to reach the ambitious goal of 5%. In the context of EW precision measurements, the reduced reach in Drell-Yan mass at LE-FCC would compromise the sensitivity to the electroweak parameters  $Y$  and  $W$  [76], bringing it below CLIC's targets.

The smaller energy would clearly imply a proportionally smaller mass reach for direct discovery at the high-mass end. Here the 100 TeV energy is an important milestone, emerging from the indirect sensitivity to new physics promised by the  $e^+e^-$  colliders through their Higgs, electroweak, or flavour programmes. The energy of a future high-energy hadron collider must be scaled to allow direct discovery of new phenomena possibly revealed indirectly in  $e^+e^-$  collisions. Most studies presented during the European Strategy symposium [74] in Granada show that 100 TeV is the energy required to achieve this goal.

Moreover, it must be stressed that not all new physics would show up via indirect precision measurements at  $e^+e^-$  colliders. Limits from  $e^+e^-$  on particles decoupled from leptons, such as squarks or gluinos, are clearly very weak. But the same can be true of weakly interacting particles. For these particles (such as supersymmetric charginos, neutralinos or sleptons), LEP itself did not set very strong limits, beyond those set by the search for direct production. For this reason, the parameters of a future hadron collider must also be tuned to push further the search for new particles just above the thresholds of future  $e^+e^-$  colliders. Even if the masses here are not large, energy remains crucial, to achieve production rates large enough to guarantee discovery, in view of the otherwise small cross sections. It has been shown in the FCC Physics CDR [2] that Wino and Higgsino DM candidates can be discovered or ruled out up to the masses of  $\sim 4$  and  $\sim 1.5$  TeV, just above the general upper mass limits,  $\sim 3$  and  $\sim 1$  TeV respectively, dictated by cosmology. A factor of two reduction in centre-of-mass energy would miss the target of conclusively discovering, or ruling out, such WIMP candidates. In a similar way, studies of the direct signals for a strong first order electroweak phase transition, documented in the FCC Physics CDR, show that 100 TeV (and  $30\text{ab}^{-1}$ ) are necessary to cover the full range of scenarios.

In conclusion: 100 TeV does represent an important energy threshold to guarantee an important set of deliverables, from the measurement of the Higgs self-coupling at the  $\sim 5\%$  level, to the exploration of WIMP DM, of the nature of the electroweak phase transition, to the search for particles responsible for deviations observed by precision  $e^+e^-$  measurements. As such, 100 TeV or more should remain as an ultimate target of the FCC programme. Lower energies would certainly allow progress with respect to the LHC, but, contrary to FCC-ee, would not add complementary information to the ultimate output of FCC-hh, and might weaken the physics case for FCC-hh by pre-empting some of the FCC-hh measurements. The cost of transiting through a lower-energy option, furthermore, would very likely add to the final cost of achieving 100 TeV, possibly jeopardizing this eventual upgrade altogether. **Altogether, the route to 100 TeV via FCC-ee seems a lot more promising.**